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ABSTRACT

A symposium involving personnel from universities and colleges, individuals from government and private agencies concerned with visual impairment, and doctoral level students was conducted to exchange information on current efforts and future needs in research with the visually handicapped child. Participants were divided into three groups to discuss goals and priorities in: organizational structures for the research community, training of researchers, and areas of needed research. Among evaluation results was that there seemed to be a general feeling that too much was planned for the symposium. (Appendixes, which make up the bulk of the document, include a copy of the initial letter, a list of participants, copies of the second letter and questionnaire, a paper titled "Childhood Visual Impairments--Sources and Uses of Knowledge" by D. Warren, an outline of the agenda, and a sample evaluation form. (SBH)

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RESEARCH DIRECTIONS IN CHILDHOOD VISUAL IMPAIRMENT

Report on a Symposium

September 14-16, 1977

The University of Michigan

Ann Arbor, Michigan

Planning Committee

Ronald Schnur
Geraldine T. Scholl, Chairperson
Carolyn Weihl
Molly Romer-Witten

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Introduction

About 26 colleges and universities, a relatively small number, prepare teachers to serve the approximately 30,000 visually handicapped school-age children spread across this country. An even smaller number, approximately ten, prepare doctoral-level leadership personnel. Because of the small number and the wide geographical dispersion, there are few opportunities for university personnel and students to confer and exchange ideas about on-going and proposed research as well as to discuss the state-of-the-art concerning research and field demonstrations. Additional problems face the professional and student in conducting research on the visually handicapped school-age population. The number of visually handicapped pupils is relatively small and scattered; their educational needs tend to become obscured by the larger and more visible populations comprising other special education groups and are neglected by the researcher. Further, the small and widely scattered numbers of potential subjects create difficulties for researchers in using conventional research designs. The scarcity of potential researchers and the small, widely scattered, heterogeneous population to be studied contribute to fragmented research efforts, limited dissemination of results, and restricted opportunities to replicate studies. Attempts to coordinate activities and to exchange information, however, must be approached on a nationwide level. This symposium was designed in an attempt to meet this need.

The purposes of the symposium were to:

1) exchange information about current research and demonstration activities, in order to appraise the present state of research in childhood visual impairment giving special attention to the resources, organizations, and orientations that exist among the community of interested researchers;

2) to determine research needs, set priorities, and develop guidelines for more comprehensive and coordinated research efforts in the future;
3) to prepare, in draft form, specific proposals for initiating research projects within the context of a coordinated and comprehensive approach.

Plan of the Symposium

Invitations to participants were sent to personnel in all universities and colleges which have Bureau of Education for the Handicapped funded doctoral-level programs (see Appendix A). In addition, selected personnel in other universities and in government and private agencies concerned with visual impairment received invitations. A doctoral level student from each doctoral training program was also invited. Affirmative responses were received from 40 participants (see Appendix B). A follow-up letter and planning questionnaire were sent to these persons (see Appendix C). The following articles were distributed to the participants:

> Urie Bronfenbrenner. Toward an experimental ecology of human development. American Psychologist, 1977, 32, 513-531.
>
> David H. Warren. Blindness and early development: what is known and what needs to be studied. The New Outlook, 1976, 70, 53-60.
>
> David H. Warren. Childhood Visual Impairment: Sources and uses of knowledge. Mimeo. (Included in Appendix D).

The Symposium opened with an invited keynote address by David Warren, Ph.D., Associate Professor of Psychology, The University of California at Riverside (see Appendix D). A reactor panel was followed by a presentation by doctoral students on current doctoral research. On the second day, the morning was devoted to a consideration of the status of some current research

projects in the field and of the question of the integration of research and practice. A copy of the Symposium agenda may be found in Appendix E.

The first part of the program was refined through small group discussions and feedback to the participants into a statement of principles and areas of concern. While the open nature of the Symposium did not include composing a formal statement, the general consensus of the Symposium is summarized below.

Summary of Symposium

Participants were divided into three groups to discuss goals and priorities in the area of research with visually handicapped children. Each group then reported the consensus for consideration and reaction by the Symposium participants meeting as a large group. While the general discussion could not fully explore the implications of all parts of the group reports, the following items were generally accepted.

The findings are reported in three areas: organizational structures for the research community, the training of researchers, and areas of needed research.

Organizational structures for the research community.

Consortia. Research consortia, composed of universities and other agencies interested in research in visual impairment, should be developed either according to geographical proximity or area of research concentration. These consortia would be ad hoc in nature and would have the following advantages:

1) a consortium would have access to a larger subject population and thus could make more efficient research use of this population;
2) a consortium would enable a number of researchers in the area of visual impairment to cooperate more efficiently in their research efforts;
3) economic benefits can accrue from an orderly coordination of research efforts using the complementary resources of several agencies;

4) vertical integration of local through national agencies may be enhanced through the expanded opportunities offered through cooperating researchers working in a consortium framework.

Communication and information network. A need was expressed for a central facility to serve as a resource or clearinghouse to which researchers and practitioners can field questions and concerns related to topics that need research. This need was expressed as part of a more general concern about the relationship of practitioners to researchers. Practitioners were seen as a source of important research questions, but lacking in the time, money and skills necessary for conducting major research. Often their important concerns are not couched in easily researchable terms. Researchers, on the other hand, often do not have the immediate sense of issues of current importance possessed by practitioners. Therefore, a communication and information network to serve as a bridge between practitioners and researchers was suggested. National agencies, such as the American Foundation for the Blind or the American Printing House for the Blind, were suggested as the type of organization that might serve as a clearinghouse between those with research needs and those with research skills.

Cost-effectiveness in research. As competition for research funding increases, consideration of the cost-effectiveness of research becomes more important in terms of probable future benefits of research.

Training of researchers

Number and scope. It was suggested that the number and scope of doctoral-level personnel be increased. The training of these personnel may take place outside university schools of education as well as within schools of education. It may not be realistic to expect that doctoral training in the area of personnel preparation can provide both expert researchers and expert trainers

of practitioners, so that special preparation programs might be needed to provide personnel specializing in research. While opportunities for such specialization now exist, increased numbers of researchers are desirable.

Post-doctoral research internships. A desirable avenue of research training lies in funding post-doctoral research institutes in a wide variety of settings not limited to universities.

Pre-doctoral training outside of schools of education. Support for pre-doctoral training related to visual impairment within a wide variety of appropriate university programs, such as psychology and sociology, is desirable.

Funding in doctoral training programs. A major impediment to the training of doctoral level researchers was identified as the low levels of available financial support to doctoral students. Suggestions were made to increase the level of support and to modify support according to needs, especially in the case of students with dependents. Since many researchers come to the field after several years of experience as practitioners, it should be expected that such students may require a higher level of support than students beginning doctoral programs soon after undergraduate study.

Research utilization as part of personnel training. College and university personnel preparation should include exposure to ongoing research activities as training in developing a cooperative relationship between practitioners and researchers, the nature and benefits of research, and mechanisms of collaboration between practitioners and researchers. Personnel in training should be made aware of their unique importance as generators of important research questions, as co-workers in the management of the research population, and as users of research results.

Research utilization in special education administration. Administrators in training should become familiar with research in much the same way as college and university personnel in

training, but in addition should have special training in applied research and in evaluation research. The nature of integration of research with ongoing programs should receive major emphasis in administration training programs.

Inservice research training for university personnel and practitioners. A strong consensus was reached that university and agency personnel, especially those concerned with personnel preparation, could benefit from inservice postdoctoral training in current research techniques with an emphasis on identifying, planning and following through on specific research projects.

Non-university agencies and research. Non-university agencies such as service agencies, government departments and private foundations should be available both to involve staff in training, to conduct research, and to provide both inservice and preservice training in research issues. These agencies can also be the sites for post-doctoral research experience, and should conduct inservice personnel preparation sessions.

Content areas for research

During the course of the sessions, several topics were emphasized as particularly suitable areas for research interest. These include:

-Evaluation of the Optacon Dissemination Project
-Rural and sparsely populated area service delivery systems
-The nature, training and competencies of professional personnel
-Family and parent-child research
-Longitudinal child developmental research.

Additional issues

Generation of questions and theory. A general concern throughout the Symposium was how, in the absence of a general theory of blindness, are research questions to be generated. The answer appeared to be in the empirical experience of practitioners

as conveyed to researchers. Difficulties in this procedure exist, however, in the organizational and training gaps between practitioners and researchers. Suggestions were made that contributions toward theory-building and the use of existing theory, such as the Piagetan, be evaluated.

Use of the research population. The population of visually handicapped children is sparse and widely distributed. Therefore, special difficulties are presented to researchers in gathering research populations. Often, children in residential schools or in certain large cities are the subjects in repeated experimental projects. The difficulties in obtaining a population, the effect on the children of repeated use in research, and the efficiency in using the available population are all questions of serious concern.

Research and jobs. Many professional positions, including those at the university level, do not emphasize research as a professional activity. Some discussion was directed at reconciling the need for research with the current demands for service activities upon the limited number of leadership personnel trained at the doctoral level.

Models for research. The marked heterogeneity of the population of visually impaired children severely constrains the use of group comparisons as a conventional research approach. Discussion concerned the applicability and use of alternative research models emphasizing small samples and individual differences.

Evaluation

As noted in a previous section, there were three major objectives for the symposium. An evaluation form was prepared to solicit reactions of the participants to the symposium. A summary of responses by role is included in Appendix F. In general the comments were favorable. Students seemed to be the most enthusiastic and found during the symposium an opportunity to meet others with the same concerns as well as to hear from

the established professionals. There seemed to be a general feeling that too much was planned for the symposium and there was not sufficient time for group discussion. David Warren's presentation was received with the most enthusiasm; the reactor panel for this session was also rated very high. The remaining sessions received favorable support with many insightful comments.

There was considerable variation among respondents regarding the achievement of the goals of the symposium. The first goal was to appraise the present state of research in childhood visual impairment. Ratings ranged from 2 to 5 with a mean of 3.6; students were the most positive and a researcher, the least. The second goal was to determine needs, set priorities and develop quidelines for more comprehensive and coordinated research efforts in the future. Again the range was from 2 to 5 with a mean of 3.5. There was considerable variation among all groups on this item. The third goal was to arrive at a proposal or recommendations for coordinating research efforts. The range was from 1 to 5 with a mean of 3.6. In summary, it would appear that the participants were reasonably well satisfied that there was progress made on the achievement of the goals.

Many participants took time to write extensive comments at the end of the form. From these observations, it was evident that the symposium had met a need for these persons. Good suggestions were made for future directions.

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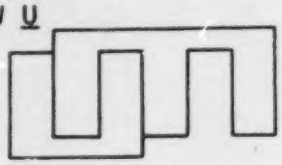
APPENDIX A



THE UNIVERSITY OF MICHIGAN

School of Education
Corner East and South University Avenues
Ann Arbor, Michigan 48109

July 13, 1977

NAME
ADD
ADD(RET)

Dear NAME:

The University of Michigan has been funded by the Bureau of Education for the Handicapped to conduct a symposium on Research Directions in Childhood Visual Impairment in Ann Arbor on September 14-16, 1977. The symposium is intended to address the need for a coordinated approach in future research in the field. The conference will have as its goals:

1. To appraise the present state of research in childhood visual impairment giving special attention to the resources, organizations, and orientations that exist among the community of interested researchers.

2. To determine research needs, set priorities, and develop guidelines for more comprehensive and coordinated research efforts in the future.

3. To prepare, in draft form, specific proposals for initiating research projects within the context of a coordinated and comprehensive approach.

The format of the symposium will include presentations with reactor panels and group discussion followed by small work sessions. BEH personnel as well as those from universities, foundations, and private agencies will attend. The symposium should provide an opportunity for researchers in the field of visual impairment in childhood to influence the overall direction, shape, and coordination of future research.

REVISED
NAME

July 13, 1977

We would like to invite you and one of your doctoral students to attend this symposium. Since an intended goal of the symposium is to encourage specific research efforts within a larger context, we ask that you prepare in advance a draft of a research proposal which can be further developed during the symposium. This proposal might take the form of a doctoral student research grant (or dissertation proposal) or a response to an Office of Education RFP for research or demonstration projects.

We would appreciate knowing, at your earliest convenience but no later than July 27, 1977, whether you and a doctoral student will attend. You and your doctoral student's travel, lodging, and meal expenses to attend the symposium will be paid by The University of Michigan. Details regarding accommodations and program will be forthcoming. In your response, please include your summer address if different from the one used for this letter and the name and address of the doctoral student.

Questions or comments can be directed to any member of the planning committee listed below.

Sincerely yours,

Geraldine Scholl

Geraldine T. Scholl, Chairperson
Ronald Schnur
Carolyn Weihl

Planning Committee

GTS:nov

APPENDIX B

LIST OF PARTICIPANTS

Edna Adelson
Child Development Project
201 East Catherine Street
University of Michigan
Ann Arbor, Michigan 48104

Lu Alonso
115-F Erickson Hall
Michigan State University
East Lansing, Michigan 48824

Edward Berla'
Coordinator of Vision
Impairment Program
Special Education Unit
University of Louisville
Louisville, Kentucky 40208

Dean Frederick W. Bertolaet
School of Education
University of Michigan
Ann Arbor, Michigan 48109

Robert Bowers
Professor, Special Education
Teachers College
Columbia University
New York, New York 10027

Nancy Bryant
Superintendent
Michigan School for the Blind
Lansing, Michigan 48906

Joan Chase
Department of Psychiatry
Rutgers Medical School
Piscataway, New Jersey 08854

Anne Corn
Teachers College
Columbia University
New York, New York 10027

Iain F.W.K. Davidson
The Ontario Institute
for Studies in Education
252 Bloor Street, West
Toronto, Ontario, Canada M5S 1V6

Allen T. Ditlman
U.S.O.E./BEH
400 Maryland Avenue, S.W.
Washington, D.C. 20202

Sherry Driggers
Department of Special Education
Box 326
George Peabody College
Nashville, Tennessee 37203

Janet Fletcher
The Ontario Institute for
Studies in Education
252 Bloor Street, West
Toronto, Ontario, Canada M5S 1V6

Emerson Foulke
Professor of Psychology
358 Life Sciences Building
University of Louisville
Louisville, Kentucky 40208

Debby Gilden
Smith Kettlewell Institute
of Visual Sciences
2232 Webster Street
San Francisco, California 94115

Carl Grube
G R 4.1
Box 688
University of Texas at Dallas
Richardson, Texas 75080

John W. Hagen
Chairperson, Developmental Area
Department of Psychology
University of Michigan
Ann Arbor, Michigan 48109

Amanda Hall
Department of Special Education
1600 Holloway Avenue
San Francisco State University
San Francisco, California 94132

Philip Hatlen
Department of Special Education
1600 Holloway
San Francisco State University
San Francisco, California 94132

Randall Harley
Department of Special Education
Box 326
George Peabody College
Nashville, Tennessee 37203

Daniel Head
School of Education
and Rehabilitation
1803 - 26 Street
University of Northern Colorado
Greeley, Colorado 80631

Daisy Hung
Department of Psychology
Post Office Box 112
University of California at Riverside
Riverside, California 92502

Jeannie Johns
Institute for the Study of
Mental Retardaion and Related
Disabilities
130 S. First Street
Ann Arbor, Michigan 48104

Gaylen Kapperman
Department of Special Education
Northern Illinois University
DeKalb, Illinois 60115

William C. Morse
Chairman, Combined Program in
Education and Psychology
University of Michigan
Ann Arbor, Michigan 481090

Mary Ellen Mulholland
American Foundation for the Blind
15 West 16th Street
New York City, New York 10011

Carson Nolan
American Printing House for the Blind
Post Office Box 6085
Louisville, Kentucky 40206

Rosemary O'Brien
Vision Services Center
Post Office Box 182
Kensington, Maryland 20795

Ronald Schnur
Combined Program - Educ. & Psychology
University of Michigan
Ann Arbor, Michigan 48109

Geraldine T. Scholl
Special Education Department
School of Education
University of Michigan
Ann Arbor, Michigan 48109

Susan Spungin
American Foundation for the Blind
15 West 16th Street
New York, New York 10011

Beth Stphens
G R 4.1
Box 688
University of Texas at Dallas
Richardson, Texas 75080

Josephine Taylor
Bureau of Education for the
Handicapped
Department of Helath, Education
and Welfare
Washington, D.C. 20202

Ron Texley
Department of Special Education
1600 Holloway Avenue
San Francisco State University
San Francisco, California 94132

Dean W. Tuttle
School of Special Education
and Rehabilitation
University of Northern Colorado
Greeley, Colorado 80631

Don Walker
Department of Special Education
University of Virginia
Charlottesville, Virginia 22903

David Warren
Department of Psychology
Post Office Box 112
University of California
at Riverside
Riverside, California 92502

Carolyn Weihl
Special Education Department
School of Education
University of Michigan
Ann Arbor, Michigan 48109

Rona Willen
College of Education
Temple University
Philadelphia, Pennsylvania 19122

APPENDIX C

THE UNIVERSITY OF MICHIGAN
Special Education
School of Education
Corner East and South University Avenues
Ann Arbor, Michigan 48109

August 8, 1977

Dear

We are very pleased that you will be able to attend the forthcoming Symposium on Research Directions in Childhood Visual Impairment in Ann Arbor, September 14-16, 1977. The first Symposium session is scheduled for 9 a.m. on September 14. We will adjourn at 3 p.m. on Friday, September 16.

Our program plan for the Symposium includes both panel discussions and work sessions. Rather than emphasizing on-going research, we plan to focus on future directions. We are purposely structuring the sessions to allow for maximum discussion and creativity. To this end, we see that one outcome of the Symposium will be a statement for publication regarding future directions research might take.

We also view this Symposium as an opportunity for researchers who otherwise might not meet in the course of their professional activities to discuss and compare their concerns. As you know, research affecting visually impaired children spans many disciplines and takes place in widely separated parts of the continent. This Symposium should enable us to share priorities and varying points of view. Thus, a second expected outcome might be the evolution of a coordinated plan for conducting future research.

Included among the participants are doctoral students who have been invited through their advisors. These university faculty and doctoral students were requested to think about and prepare, in draft form, a proposal for some research project. A third expected outcome should be the opportunity to influence such research efforts now in the planning stage.

August 8, 1977

In order to achieve our objectives, it is necessary that each participant assume an active role in the Symposium. We want to know your current research interests so that we can plan the program around the varying areas of expertise and activities of the participants. We ask you to complete the enclosed short questionnaire and return it to us as soon as possible.

We look forward to seeing you. Please do not hesitate to call us with any suggestions or questions that you may have.

Sincerely yours,

Geraldine T. Scholl

Geraldine T. Scholl, Chairperson
Ronald Schnur
Carolyn Weihl

Planning Committee

Enc: Questionnaire and return envelope
List of participants as of 7/31/77
Campus map

GTS/dmm

QUESTIONNAIRE

Please return as soon as possible to:

Geraldine T. Scholl, Chairperson
Symposium on Research Directions in
Childhood Visual Impairment
2022M SEB - The University of Michigan
Ann Arbor, Michigan 48109

Name:

Address:

Affiliation:

Title:

Social Security Number:

Airline and flight number with date and time of arrival:

Do you wish any change in your hotel reservations?

Please describe briefly your current research interest as it relates to children with visual impairments.

Are you planning to prepare a draft of a dissertation or research proposal? YES___ NO___

If yes, please provide a short paragraph indicating the topic of research.

Please give us any comments or suggestions you have regarding the program.

QUESTIONNAIRE

Please return as soon as possible to:

Geraldine T. Scholl, Chairperson
Symposium on Research Directions in
Childhood Visual Impairment
The University of Michigan
2022M School of Education
Ann Arbor, Michigan 48109

Name:

Address:

Affiliation:

Title:

Please desciibe briefly your current research interest as it relates to children with visual impairments?

Are you planning to prepare a draft of a dissertation or research proposal? YES___ NO___

If yes, please provide a short paragraph indicating the topic of research.

Please give us any comments or suggestions you have regarding the program.

APPENDIX D

draft copy - Warren

CHILDHOOD VISUAL IMPAIRMENT: SOURCES AND USES OF KNOWLEDGE

The concern of this conference is research. We are gathered here to look ahead, to consider the ways in which we might best do research on childhood visual impairment. Research is the source of much knowledge, and therefore this assignment becomes one of determining the methods by which we should acquire our knowledge. But research is not done in a vacuum, it is done for a purpose. It seems appropriate, then, to begin not by talking about research but by considering the purpose, the reasons for doing the research at all. If we can first define the uses to which we wish to put our knowledge, then we should be in a better position to formulate plans for gathering that knowledge in such a way as to make the knowledge useful.

There are, after all, different kinds of knowledge, and there are different uses of knowledge. Any given body of knowledge may be more or less appropriate for a given use or set of uses. Let me contrast two very different research approaches, both from psychology, each with its resulting kind of knowledge. On the other hand, we have what is known as differential psychology, or the psychology of individual differences. The goal of the differential psychologist is to be able to account for the differences between people, their personalities, their abilities, their characteristics. The differential psychologist takes it as a given that people differ in important ways, and he sets out to chart and to explain the differences. With this as the goal of his body of knowledge, he structures his research to be maximally sensitive to differences. On the other hand, we have another kind of psychologist who is concerned mainly with similarities among people. His goal is to construct models of functioning in such a way

that they can describe the human being-- describe how perception occurs, how memory works, how people learn, and so on. He searches for the ways in which people are alike. His search for rules for all people makes him despise variability -- it is his worst enemy. Therefore his research is designed to minimize variability. He is oriented to statistical means, and he considers variability to be a nuisance -- in fact, he calls it "error."

These two psychologists, one exploring differences among people and the other exploring similarities, use very different research paradigms. They choose different research tools because they seek to generate different bodies of knowledge, ones directed to different purposes.

This particular distinction happens to be germinal in considering research on visual impairment, and I will develop it more later. But for now let it simply serve as an illustration that there are different kinds of knowledge systems, and there are research tools appropriate to each kind. My point is that while we can do many different kinds of research, and therefore construct many different bodies of knowledge, if we can define the uses to which we wish to put a body of knowledge, then we'll be in a better position to define the kinds of research that we need to use to generate the appropriate body of knowledge.

This definition of purpose requires a great deal of wisdom. We must not define the body of knowledge too narrowly or too broadly. It must be broad enough to encompass not only the specific uses that we can identify, but also those that we don't happen to think of but that will arise in the future. If we define the quest too broadly, though, we run the risk of insufficient specificity. We'll have general ideas about what's good and what's bad for the visually impaired child, but we won't be able to pin down

answers to specific questions in sufficiently operational terms to be of any real use.

This job of defining a set of purposes, and thereby setting implicit guidelines for our body of knowledge and the methods to acquire it, seems like a tall order, and indeed it is. Let me take a few minutes to suggest my biases. Perhaps some of you will not agree with even these broad strokes. I don't offer my suggestions as more than a starting point for discussion, but I do think that such discussion should be part of our task in this conference. The important thing is that we realize the need to consider not just specific research plans and formats, but that we consider the rationale for choosing them and indeed the very purpose for doing the research in the first place.

I will propose, very broadly, that our body of knowledge should let us understand of the development of visually impaired children well enough to do the following:

1. provide each visually impaired child with an environment that will optimize his development, that is, allow him to develop to his fullest capacity in every area of human functioning;

2. as a corollary, allow the visually impaired child to avoid the various developmental pitfalls -- inordinate delays in developmental progress and inordinate shortcomings in eventual developmental level;

3. since we will undoubtedly be unsuccessful to some degree in accomplishing these purposes, our body of knowledge should allow us to help the child who does show developmental aberrations to overcome them and to resume an optimal developmental course.

In short, our purposes should be to set the stage for optimal development and to allow the effective repair of non-optimal development.

As broad as these stated purposes are, they nonetheless place some very specific requirements on the body of knowledge that we must seek. First, we must have some yardstick, some criterion that we may use to determine whether any given child is realizing his optimal development. This sounds like descriptive age norms, but it is nothing so simple. It would be relatively simple, except that as our practitioner cousins tell us, visuall impaired children vary tremendously in their actual abilities and characteristics, and presumably in their potential abilities and characteristics. We do need yardsticks, but we need them to be sufficiently sensitive to apply to individual cases, not to across-the-board statistical means. Visually impaired children are indeed individuals, and the success of our body of individuals, not as undifferentiated members of a homogeneous population.

Second, the acquisition of a fully sensitive normative developmental sequence is not enough. It is at best only an evaluative device, and it cannot inherently tell us anything about the causality of development. It is in causality that the true use of an applied discipline lies. And make no mistake about it, ours is an applied discipline. No matter how basic or applied we may think of any given piece of research, ultimately the test of the body of knowledge lies in whether it can be used effectively. That means searching for causality, for the reasons for optimal or non-optimal development. And given that we do not choose to conduct unethical experimentation, I suggest that the key to investigating causality lies in the study of individual differences. We may not do the experiments, varying the situation and evaluating the outcome and drawing causal conclusions. But a good analog of the experimental approach lies there for us, ready made. The conditions under which visually impaired children are reared surely encompass all the

experimental variations that we might ever dream of. Children with apparently identical potential develop in vastly different ways and to different levels. There are reasons for the differences, and we must search for them. This is our hope for the development of a body of knowledge that has causal properties.

Where do differences between individuals come from? One of our favorite debates concerns the relative contributions of genetic and environmental influences on development. I don't propose to resolve or add to that debate, but I do propose that we can do a lot more with the environmental side of it than we have done. I'm not suggesting that environmental influences are more important than genetic ones, but I think that there are two reasons that it is in the area of environmental influences that we should invest a great deal of effort. One reason is that environmental influences are much more accessible to the researcher, and the other is that the environment is under our potential control to a much greater degree, at least in the near future. For both reasons I urge a far greater thrust of research into the effects of the environment, broadly conceived, on the development of the visually impaired child. This is my third point.

We developmental psychologists have been talking about the importance of early experience for a couple of decades now. Our conviction that the environment is important stems from a line of research with animals begun in the late 1940's, research that explored the effects of particular rearing circumstances on various kinds of development in various animals. Because of humane and ethical constraints, we don't have any such detailed knowledge about the effects of particular environmental circumstances on the development of humans. Yet we claim that the environment is important, and we're undoubtedly correct in our claim.

I don't believe that we need to remain in the realm of speculation about the developmental effects of early experience. Again, I think the research answer lies in the study of individual differences. The analogs of the experimental conditions exist. Children are reared in vastly different circumstances. We need only to study these variations effectively in order to develop our knowledge about environmental effects. And we should do so with a great deal of energy, because it is in our potential structuring and restructuring of the environments and experience of visually impaired children that we may bring our body of knowledge to bear on the goal of having every visually impaired child reach his optimal level of functioning.

Fourth, I propose that our body of knowledge must become much more fully integrated than it is now, integrated in two ways. First, we must realize that development in one area, such as perception, does not proceed independently of development in other areas, such as cognition, or language, or socialization. We tend to isolate these areas, partly because we tend to identify our areas of concern along compartmentalized lines and partly because research is conceptually and procedurally easier to do if we keep it segmented and constrained. But this segmentation will not produce a body of knowledge that is oriented to the whole child. It will not allow us to predict, for example, what might happen to social development if we introduce a cognitive training program for two year old congenitally blind children. Yet we have an obligation to know what will happen, if we wish to intervene in such ways.

I will call this first sort of integration horizontal -- horizontal because we seek to integrate our knowledge across the various aspects of development. I will characterize a second necessary kind of integration as vertical. Take mobility in the spatial environment as an example. There are all sorts of skills involved in mobility, skills that are hierarchically

interrelated. There is the very conceptualization of space, there is the ability to plan and carry out locomotor activities, there is the ability to perceive auditory, tactile, and olfactory cues and to relate them to body movements, and so on. We have research on each of these levels, but we have relatively little research that involves several or all of the levels at once. Vertical integration in the fase of mobility means the simultaneous study of a whole set of hierarchically related skills. We have, at present, a very choppy picture of what mobility really involves. I think that we can fill in thepicture much more clearly if we seek vertical integration in our body of knowledge.

So far, I've suggested that we might consider the goals, or uses, of our body of knowledge to be first the ability to structure the environment of visually impaired children in such a way as to optimize the development of each child, and second the ability to provide remediation when development goes awry. Further, I've proposed that a body of knowledge that hopes to serve these uses must provide an adequately differentiated picture of what normal development is, must be concerned with the causality of development, both normal and abnormal, must contain a substantial orientation to the effects of experience and environment, and finally must be well integrated both vertically and horizontally.

How, then, do we go about producing such a body of knowledge? What sorts of research should we do? First, I'd like to touch on the issue of applied and basic research. Let me reiterate my proposition that the uses of our body of knowledge are applied uses. In this respect, the body of knowledge that we seek has greater demands on it than one that is sought purely on grounds of armchair interest. There are certainly many very interesting branches of science that have no apparent short, or even long term applied

significance. Our branch does have these applied aspects, and indeed it is largely the need for effective applications that justifies much of our research. But I will maintain vigorously that the fact that the goal of the body of knowledge is applied does not mean that applied research is the single route to acquiring the knowledge. This is a most important distinction to make.

Let me clarify this point. We think of applied research as research that is conducted to provide a solution to an immediate and specific problem. Certainly we need such research. But applied projects should not be construed too narrowly. At the same time that they may answer a specific question, they may also contribute to the larger body of knowledge, by being developed, designed, and interpreted within that body of knowledge. By virtue of being done with an orientation to the larger body, they will add to it.

On the other hand we use the term basic to refer to research that is done with the primary goal of contributing to the general system of knowledge, without being constrained by the nature of an applied problem that needs an immediate solution. We need such research desperately, for it more than purely applied research is the skeleton, the integrating structure, for the entire body of knowledge. A body of knowledge that is useful in meeting our purpose is one that is integrated, a system such that we can consult it and know, in advance, what is going to happen at point A if we pull a little thread at point Q or poke a hole at point X. A compendium of specific applied research finding will not tell us this, and it will not let us truly understand the visually impaired child as an integrated and whole person.

Let me make a plea to the representatives of funding foundations and agencies not to neglect the vital need for basic research. Such a neglect would be tragically short-sighted. Although some very applied research projects may have more short-term impact and more tangible return on the

research dollar, applied research is not by itself the key to the needed body of knowledge. Nor is basic research by itself the key. But basic research is vitally important. Researchers should not have to bootleg basic research projects, stealing them out of spare time and left-over budgets. Good basic research deserves a strong status and a strong basis of financial support. Resources must be made available for basic research that will contribute to the generation of an integrated system of knowledge, for without that integration, the body of knowledge will be fragmented and incapable of generating effective solutions to present and future problems.

Perhaps the terms basic and applied are useful, although I wouldn't be unhappy to see them die of disuse. I certainly hope that we can make the distinction unnecessary, first, by making sure that research that is primarily oriented to a specific problem is done in such a way as to enhance the system of knowledge. Second, I believe there are some major reorientations needed in our more basic research, and I'll get to them in a moment. But above all, we should all avoid generating two different bodies of knowledge, one the catalog of applied findings and the other the framework of basic research. We cannot affort a dual system -- a single system can contribute to our overall purposes far better than can the sum of two separate systems.

How should we reorient our more basic research? One of the most popular research models in the psychological literature is oriented to the statistical mean -- using this model, researchers evaluate performance, comparing the means of two groups to see if they differ from another on an identical measure, or comparing the same group under different circumstances to see if it changes as a function of the circumstances. The success of this method lies in its ability to find differences between groups or between conditions. If statistical

means differ, then we have positive results and we can report them. If means do not differ, then we have negative results, and we cannot report them, or usually we cannot. One of the reasons that we sometimes fail to find a statistical difference between means is that there is too much variability among the scores. The greater the variability, the less likely it is that a significant difference between means will emerge. So the user of this research model tries to reduce variability, for it is his worst enemy -- he calls it error variance. The more successful he is at keeping it to a minimum, the more likely he is to find significant differences, or positive results, and thus the more likely he is to publish and advance his career. Small wonder that he despises variability.

Variability has also plagued many people doing research with the visually impaired, particularly when their goal is to compare various groups doing the same thing or a single group in different conditions. Variability tends to reduce the statistical significance of results, to make apparent differences into non-significant ones. In fact, it often seems, to researchers who work with both visually impaired and sighted samples, that variability is a worse nuisance in research with the visually impaired than with the sighted -- there always seems to be a wide range of scores on any measure, and thus variability is higher and fewer differences emerge than in similar work with the sighted.

These days we rarely see a study in which children who are blind from birth are grouped together with those who became blind later on. The conceptual reason for making this distinction is that we realize that there are important ways in which these two groups of children differ. The statistical reason is that because of those differences, there would be a

great deal of variability around a mean introduced by failing to distinguish between subjects on this basis. If we did group the children together, we would find that high variability, and we would call it error variance. Error variance -- the statistical term is chosen to represent the assumption that variation around the mean on some dependent measure is simply a result of error, perhaps measurement error of some sort, but that it is not lawful. But in our example of the two kinds of blind children, if we grouped them together, we know that some of the variability that we'd introduce is lawful -- it is variation that we could identify if we structured the research differently.

I believe that much of the variability that is found in studies using visually impaired subjects is not what we should properly call error variance. It is not lawless, but rather it is variation that is introduced into dependent measures by the fact that there are many, many variables that affect our dependent measures. Not just whether a subject was blind from birth - that is only one source. But etiology of the impairment. Amount of residual vision. Early auditory experience, early motor experience. Early social environment. Opportunities for reinforced learning. Availability of useful social services. Parents' reactions to the fact of the child's impairment. We could go on and on. For any given dependent measure, some subset of these variables are undoubtedly creating variance in our results, and this is variance that we normally call error variance. But I think we're not doing enough with our research if we are satisfied to call it error variance and forget it. There is a lot of it that we can track down and study; we can discover the laws that govern the ways that all these variables affect the dependent measures that we're interested in, the ones that represent how visually impaired children function. We'll still have something to call error variance, partly a result of measurement error or unreliability of

measures, and partly a result of variables that we haven't succeeded in tracking down yet. That very characteristic of visually impaired children that we find so aggravating, their variability from individual to individual, is, I maintain, what we should be studying. We should not forget about the means -- they are interesting and informative as summary statistics. But the really interesting thing is variability, how people differ, and why.

And I'm not suggesting that we study individual differences just because they are more interesting than statistical means and differences between means. Individual differences are critically important for a body of knowledge that has applied demands. Applications are conducted with individuals, not with statistical means. If we want to generate a body of knowledge that will serve to let us structure the experience of each visually impaired child so as to optimize his developmental potential, then we will have to explore individual differences. We will have to come to understand those variables that I mentioned, and all their cousins, because it is just those variables, the ones whose effects we short-sightedly call error variance, that we have the potential to affect in applied settings. But we'll only discover how they work if we study them effectively.

To summarize, if we take as the uses of our body of knowledge something along the lines of what I proposed, then the body of knowledge has to be heavily oriented to individual differences rather than to statistical means. That sort of body of knowledge, in turn, requires research that is oriented to individual differences, not to group means.

I think there's another instructive angle to this issue of research types. From the beginning of my own experience with visual impairement, talking to researchers and reading their work, and talking to practitioners, I have

been struck, and dismayed, by the lack of regard that many practitioners have for research. They say: what good is that research for my purposes? I have to deal with individuals, and the research doesn't tell me how to do it. Why should I be familiar with research? Why should funds that could be supporting applications be diverted to research that doesn't do me any good? My first reaction to this attitude was to downplay it by concluding that the practitioner who talks this way doesn't really understand basic research, doesn't understand theneed for developing a broadly based system of knowledge rather than a set of specific applied questions and answers. Now I think I was wrong in that interpretation. I think the practitioner has a point. Much of the available research does not, indeed, have any direct bearing on what the practitioner does or might do differently. Not only that, though -- much of the available research is not designed, interpreted, or communicated in such a way as to contribute to a body of knowledge that will be useful to the practitioner <u>even in the long run</u>. What it will give us, over the long run, is a picture of the average visually impaired child, perhaps with some major differentiations such as that between congenitally and later impaired children. But even the most exhaustive and accurate picture of the average visually impaired child will not be useful to the practitioner. He needs to know about the effects of those variables that we've been neglecting, whose effects we've been ignoring under the general heading of error variance. The long term goal of our research should be to generate a body of knowledge that includes those variables and how they operate. And that goal requires research on individual differences, not on statistical means. We can provide the practitioner with a much more useful body of research than we have done, but it will involve doing a different sort of research, research that explores variability rather than ignoring it.

A research model that has been used very extensively in the past is that which compares visually impaired with sighted children. There seem to be two distinct reasons behind the use of such a comparative model. One is to study visually impaired children as a so-called "natural control group," against which to compare sighted groups. The other is to use sighted characteristics and levels of functioning as a yardstick against which to evaluate visually impaired children. I will argue that both of these kinds of comparison are invalid to a great degree.

Take first the issue of using visually impaired samples as controls for sighted samples. The rationale is usually this. Some function, some kind of behavior, is hypothesized to involve visual organization, or to rely on a visual substrate. The behavior, for example some kind of tactual or auditory information processing, is studied under various conditions with sighted samples, perhaps assessing performance blindfolded or in the dark. So far, so goo. But then, as if to apply the ultimate test, a group of blind subjects is studied, often congenitally blind, under the reasoning that such subjects have never had any visual function and therefore their tactual or auditory performance should represent performance with no possibility of visual involvement. Let me quote from such a study. First, from the sighted results: "These data suggest that a visual image of the hand was elicited when the finger was stimulated tactually." Then, "If the visual image interpretation is correct, congenitally, totally blind children, who have no visual image of the hand, should make more errors in . . [certain conditions]". Conceptually, this kind of reasoning seems clean enough. But I argue that it is basically misleading in attempting to shed light on the functioning of the sighted sample. In fact, this kind of comparison violates the most basic principle

behind the experimental method, Mill's Method of Difference. That principle states that "if two situations differ in only one respect, and an effect is observed in one situation and not the other, then the effect is due to the factor which is different" (Plutchik, p. 179). In this case, the differing factor is the presence of absence of vision. Certainly sighted children have it and congenitally, totally blind children do not. The difference is there. But are they really equivalent in all other respects, as the method of difference demands? I argue that they are not. The evidence from the developmental literature suggests that blind and sighted children differ substantially in the course of their motor development, in their modes of learning, in their cognitive development, in their course of socialization, and so on. With such a pattern of developmental differences, where is the equivalence demanded by the method of difference? I don't think it is there, and therefore I propose that the experimental rationale behind this direct and simple comparison of visually impaired and sighted groups is certainly too naive, and probably in some cases downright misleading. The issues are far more complex. Vision may indeed be a major differentiating factor, but it is by no means the only one and perhaps not even the major one.

Perhaps the most important consideration in the direct comparison of blind and sighted samples on identical tasks has to do with the ecological validity of the tasks and of the behaviors which the tasks are designed to represent. The same experimental task may tap a behavior that serves a very different ecological function for visually impaired and sighted people, and thus the same task may test behaviors which are the same on the face of it but very different in the role that they serve for the person. The comparison would be invalid or misleading with respect to the goal of acquiring a real

understanding of the functioning of either visually impaired or of sighted people.

The other side of this visually impaired - sighted comparison coin is if anything probably more complex. When should we compare visually impaired children to sighted children for the purpose of deciding something about visually impaired children? This question quickly takes on aspects that I prefer to avoid, questions such as the very basic one of whether the visually impaired child should be considered simply as an ordinary child who happens not to be able to see, or instead as a special child with a special set of characteristics, abilities, and perhaps disabilities. This question has been debated at length -- my view, briefly, is that in any such form the issue is vastly oversimplified, and that any simple answer is bound to be inadequate.

But these large issues aside, there are definitely some implications for research that should be considered. Let me discuss some of these implications by using several examples. A good one to start with is intelligence. Let us define intelligence, for our purposes, as the capacity to adapt to the demands of the environment. I'll start with a blunt conclusion: it is impossible to make any really meaningful comparison of the intelligence of visually impaired and sighted children. Why? Basically because of the incomparability of the measuring devices. Tests for sighted children rely heavily on vision, both for content and for administration. Sometimes the visually dependent material is simply omitted and the remainder of the test is administered in this modified form to visually impaired children. This procedure is inadequate for the visually impaired, since the meaning of any scale depends on the composite contribution of all the items tested on the norm group, and any omissions may change the meaning in unknown ways. Tillman and his colleagues (Tillman, 1967 a, b; Tillman & Bashaw, 1968; Tillman & Osborne, 1969) have presented several analyses

that demonstrate the extent of the difference in patterns of visually impaired and sighted children on the verbal scales of the WISC. These analyses demonstrate that even when the total verbal IQ scores are the same, the subscale patterns of visually impaired and sighted children differ considerably. Thus I question whether the test can be considered to tap the comparable abilities in the two groups.

The performance scales from sighted IQ tests are normally omitted when testing the visually impaired child; only the verbal scales are administered. This approach is probably misleading, since the capacities assessed by the verbal scales are considered to be only a part of the thing we call intelligence. Performance scales that may be used tactually with the visually impaired have been developed, and they have been normed to some extent on visually impaired samples. But these tests are not necessarily valid when administered to blindfolded sighted children, since sighted children do not ordinarily perform tactual tasks without vision and may therefore perform at a depressed level.

In short, it is extremely difficult, if not impossible, to make direct comparisons of blind and sighted children on identical instruments if those instruments adequately cover the range of abilities considered to be part of the overall notion of intelligence.

Even more important than these technical issues, however, is the fact that the comparison of the intelligence of visually impaired and sighted children if of questionable significance in the first place. Intelligence, again, has to do with the ability of the individual to adapt to, or get along in, his environment. Certainly the environments of visually impaired and sighted children are different, and thus the adaptive needs are different. The

question of whether adaptation is equally adequate cannot be approached by simply administering a common test. The argument has been made that sighted IQ norms are perfectly appropriate for the visually impaired child, to the extent that we consider that he should be expected to adapt to the sighted environment. That expectation may be valid enough, but still, the visually impaired child's constellation of ways of adapting to that environment are considerably different than the sighted child's constellation, and thus a standardized test cannot possibly have equal criterion validity for both children.

I don't mean to imply that the issue of adaptability in the visually impaired child is not important -- it certainly is. But our concentration should be on the search for those factors that lead to better adaptability in some children than in others, and in particular what we may do by way of structuring the visually impaired child's environment so as to elicit the development of an optimal capability for coping with the demands of life.

There is another comparative issue that I wish to apply across all areas of development. The issue is that of developmental lag. Developmental lag refers to situations in which the visually impaired child is found to develop at a slower rate than the sighted child. So lag involves the visually impaired - sighted comparison intimately, and lag may be considered in any area of development where there is some orderly and age-related progression of events, such as perceptual-motor behavior, cognitive skills, socialization, and so on. Let me use an example from the perceptual-motor area to illustrate. Fraiberg and others have documented impressive developmental lags in manual reaching and in locomotor behaviors such as crawling and walking, and Fraiberg has argued persuasively that the lags in reaching and in locomotion are intimately related to one another. She has also reported that despite numerous

attempts, her researchers have been unable to induce reaching and locomotion any earlier than they occur spontaneously in the congenitally blind child.

Let us suppose that there is indeed some as yet undiscovered means by which we could induce earlier reaching and locomotion, some experiential program which would allow congenitally blind infants to show those behaviors on the sighted timetable, with no lag. I would want to question the wisdom of using such a program. Presumably it would involve intervening quite substantially in the blind infant's experience over an extended period of time. What other set of experiences would that infant have to give up, in order to avoid the reaching and locomotor lags? There is a lot for the infant to do in the first year, in the areas of cognitive development, socialization, language, and so on. What would be sacrificed in order to avoid these developmental lags which, so far as we know, seem to be absolutely normal for the congenitally blind infant? Perhaps what Fraiberg's impressive evidence should tell us is that here is an area where we need not be concerned, rather than one into which we should pour a lot of energy. There are many congenitally blind adults who have impressive locomotor skills -- presumably they showed these developmental lags, and so there must have been other factors which led them to acquire the skills that they have. Our research should now be directed to finding those other factors, not to finding ways to avoid a particular developmental lag.

Generally, my point is that our definition of "normal" for visually impaired children should be based upon the study of visually impaired children, not upon what is normal for sighted children. And perhaps this means that the whole notion of developmental lag, inasmuch as it involves comparison with sighted development, is basically misleading.

Enough examples, I trust that my bias against comparisons of blind and sighted children is fully evident, and that the reasons behind the bias are persuasive. I do not think that the comparisons are justified, for the most part, either for the purpose of comparing the sighted with the visually impaired or for that of comparing the visually impaired with the sighted. We will do better, in our quest for a body of knowledge about visual impairment and development, to study visually impaired children and to study the variables that produce differences among them.

Now it's time for me to be more positive, more constructive. I have argued that the traditional models for research that involve an orientation to the statistical mean and comparisons of visually impaired and sighted samples are not the appropriate ones for the future. I do have an alternative to offer.

The model that I think will produce the most useful research for the development of an effective body of knowledge is one that I like to call a hierarchical or multi-level model. The most basic principle behind the model may be stated simply: research on a characteristic should involve not only the evaluation of that characteristic, but should also include attention to the etiology of that characteristic, and to the ways in which the characteristic is expressed in real-world functioning. We may think of these three categories as levels, <u>first</u> an <u>etiological level</u>, or where the characteristic comes from and what influences it, <u>second</u> an <u>evaluative level</u>, which is what we normally do when we use some experimental task or evaluative test, and <u>third</u> a <u>functional level</u> having to do with function in the real world. I think that our concentration has been overwhelmingly on the middle level, the evaluative one, and that we have relatively neglected the other, bracketing levels that have to do with

where the behaviors and characteristics come from and what they depend on, and how the behaviors and characteristics affect the individual's functioning. Further, I urge that all three of these general levels be included within a given study whenever possible. We shouldn't have one group of researchers concentrating on etiology and contributing factors, another on developing and administering indicator tests, and a third on functional expression, but rather each study should include all three levels, on a common group of subjects.

Let me give an example or two. A study of a cognitive ability, such as a classification skill, should not measure just that ability by some indicator test. It should include evaluation of present and past factors that might produce variations in the ability, such as contributing sensory and perceptual factors and characteristics of the child's learning environment both past and present. It should also include evaluation of the ways in which the ability is expressed in the child's behavior, such as his ability to understand his world and to succeed in school. A second example: much of the available research on personality has concentrated on characterizing the patterns of difference between visually impaired and sighted children, finding out how their profiles differ on some standardized test of personality. The study of personality should not stop there, though. It should search for factors that may have led to variation in the measured personality characteristics -- that is the etiological level --, and it should assess the relative advantages or disadvantages that the characteristics confer on the subjects in adapting to various real-world situations.

These are examples of how the hierarchical model may be applied. The key is to avoid simply studying indicator behaviors or characteristics in

isolation from their causes, on the other hand, and their effects on the other hand. It is clearly a more complex research model than what most of us are used to working with. It is clearly one that requires a multivariate approach. We cannot think in the simple terms of a single independent variable and a single dependent variable. Such simple models are misleading -- behavior is complexly determined and complexly expressed.

Not surprisingly, admitting the necessity of studying complexity adequately leads back into the issue of individual differences. We must study variation, if we are to develop an adequate body of knowledge, and if we want to study variation, when we must study its cources and admit to the complexity of causality. Earlier I stressed the need for a vertically integrated body of knowledge -- the hierarchical research model is designed to generate vertically integrated knowledge.

At the same time, I think that an admission of the complexity of behavior may lead us toward a more effective consideration of the whole child, and this is the issue of horizontal integration of knowledge. The various areas of development are not isolated in real children, they are only isolated in research. If we can develop research models that will make us consider complexity adequately, then we can work away from this isolation. Take an example in social development. The emergence of social attachment, the narrowing of emotional bonds to a select few people in the first year of life, clearly depends on social factors but just as clearly on cognitive factors. The infant must develop the concept that there are d fferent people with different identities, and this concept in turn depends on the so-called constancy concept, the concept that a person or object that is present in one instance is the same person or object that is present in another instance. We won't be able

to understand the etiology of social attachment without considering the development of these cognitive skills. If we do consider these cognitive skills and how they differ among children, we'll be in a better position to understand the process of attachment itself and why the course of attachment differs from one child to another. It is only in such larger contexts that we will really acquire a picture of development. The hierarchical model, with its emphasis on the broad range of etiological factors, lends itself to the development of such a picture.

That's the crux of what I propose for our future research formats. There are several additional points that I would add, though. I've already stressed the need to consider causality, and I would again like to stress the need to evaluate environmental factors as part of the causal picture. We simple haven't put enough emphasis on how the child's physical environment, his social surround, his learning environment, his perceptual experience, affect his development. These factors are important elements of the causal picture, and we can build them into the hierarchical model once we develop adequate ways of measuring and representing them. Another very important point has to do with so-called "status" characteristics such as visual history, severity of impairment, etiology of impairment, and so on. These are factors that the student of sighted development does not normally have to consider, but they are factors that are of immense importance in research on visually impaired children.

In reading the literature on training programs of various sorts, or on attempts at remediation of various "problems," such as emotional problems or cognitive lag problems, one is struck by the absence of reports of failures. One reads primarily of successes. In the emotional disturbance area, for example, we have reports of successful interventions based on psychoanalytic

principles, on learning theory approaches, on educational approaches, and so on. We don't read that these or other approaches fail, for the most part. I don't believe, in this case, that the published literature is representative of the research. Journals want to publish successes, and usually this policy is justified. And workers want to report their successes, not their failures. This is all understandable, but I think it may be retarding of progress. We cannot really afford resources to recreate one another's failures. But if the failures, and I'm sure there are many, aren't reported, then they will be recreated, because others will have the same ideas and try them out. And so we'll waste resources.

Actually, the issue is more complex. Usually neither failures nor successes are unqualified. I make another plea here for consideration of individual differences. If some remedial approach does fail, it usually fails "on the average." Considering individuals, there are both successes and failures. The same is usually true of approahces that are reported as successes -- they are successful on the average, but there are some subjects who are untouched by the procedures.

The purist may want to consider this variability to be random variation. If the approach produces an average effect that is sufficiently positive, considered against the variability of the change scores, then the approach is characterized as successful. That it didn't work for some of the sample is considered an accident. But again, I don't believe that all variability around a mean is accidental, and I maintain that we can learn a great deal from it. A given approach may not work for some people for perfectly lawful reasons, because they have some set of characteristics that prevent it from

working, or working so effectively. What we should do, I maintain, is to study the individual successes and failures of a given approach for clues as to why the approach did work for some and did not for others. Stated another way, what characterizes those for whom the approach did work, in contrast to those for whom it did not? If we can find some set of distinguishing characteristics, then we can choose where to apply that approach and where not to.

I'd like to make another point, closely related to this one, and I'll continue with the example of emotional disturbance. We read, for the most part, case reports of children who are emotionally disturbed, together with attempts to pin down traumatic experiences or constellations of unfortunate environmental characteristics. These reports are valuable, for they tell us what to avoid. But the picture could be made far more complete by corresponding analyses of some of the many children who are not emotionally disturbed. What was their environment like? What did their parents do? What sorts of services did parents and children have available? Reasoning is difficult when it is based only on negative instances. Our goal is the positive instance, and what better way is there to reach it than knowing about those positive instances and what they have in common.

Since the major objectives of the conference include not just general orientations to research but also specific plans, I would like to suggest a piece of research that I believe should receive a high priority. The study is a longitudinal one, of visually impaired children beginning in early infancy and continuing until at least five years of age and preferably until eight. There should be a substantial sample, on the order of 150 to 200 children. The sample should include children who are visually impaired at

birth, and these children should be enrolled in the study as early as possible, certainly by a month or two of age. It should also include children who become blind sometime after birth, possibly as late as four years, although the extension of this time variable increases the scope of the study considerably. The subjects should be unselected with respect to etiology and extent of impairment, but careful records should be kept of such variables and they should be built into the battery of independent variables for analysis.

The scope of the study should be broad, covering aspects of perceptual-motor, cognitive, language, social, and personality development. The study should seek to create a picture of the whole child, considering the multitude of interactions among these various areas of development. The study should place almost as much weight on the assessment of the subjects' environments as on the assessment of the subjects themselves. Finally, and to no one's surprise, I would want to see a substantial, indeed a primary, concentration on individual differences. The goal should be not only to chart development normatively, but to trace differences in development that are attributable to status variables and to variations in a broad range of environmental characteristics.

Let me briefly try to justify the need for such a study. I am struck by the gap in our research knowledge between the ages of about two and six. There is very little in the literature about the development of preschool children in this age range, and yet we developmental psychologists believe that this is an immensely important developmental era. We could, of course, do a lot of cross-sectional research on this age range and hope to fill the literature gap, but I think that the longitudinal approach, while perhaps more expensive and time-consuming, has very important advantages. It would let

us integrate we know about development in infancy with what we know about school age children by tracing a known sample throughout the intervening years. We would not have to wonder whether our samples were comparable at the various developmental levels, as we always must with cross-sectional research. The longitudinal approach would allow us to collect much more information about each of the subjects, information that would allow the effective use of the hierarchical research model. In addition to studying the subjects thoroughly, we could study their environments thoroughly, moreso than we could in cross-sectional research. The longitudinal approach would also help clarify the significance of such typical developmental lags as that in locomotor behaviors. Do such lags find expression in later abilities in some lawful way, or do the lags gradually disappear? Looking at the issue from the other end, a longitudinal format would allow us to look at our group of six year olds, ascertain which of them have better or worse mobility skills, or cognitive skills, and search for patterns of difference in their early experiences that may account for the later differences.

It is clear that what I am suggesting is a major effort, one that cannot be conducted by a single researcher or in a single location. It will take the combined expertise of several investigators to design the measures in suitable ways, and it will take several locations to generate the sample needed for meaningful multivariate research. Nor will the study be done on a shoestring -- it will be expensive and time-consuming. It may be that several funding sources will have to cooperate with one another to make the study feasible. I urge that whatever the source, the funding should be guaranteed at the outset for the duration of the study, perhaps for an eight year period.

This is a tall order, but it has the potential for tall rewards. I think that if it is done well, this research project would contribute immensely to the establishment of a broad developmental picture of the visually impaired child, or better, of visually impaired children in all their variety. It should allow, through careful evaluation of the environment and through the careful use of multivariate data analytic techniques, the establishment of a picture of development and what affects it positively and negatively. I certainly don't mean to imply that this sort of longitudinal study is the only research we need at this point, or that it will answer all the questions that we need to have answered. But I do think that such a study, well done, would serve as a valuable basis for integration of much of our otherwise fragmented knowledge, as well as generating important new information.

APPENDIX E

SYMPOSIUM ON RESEARCH DIRECTIONS IN CHILDHOOD VISUAL IMPAIRMENT

Sponsored by The University of Michigan, School of Education, Special Education Program and partially supported by a grant from the Bureau of Education for the Handicapped, U.S. Office of Education, Department of Health, Education and Welfare.

September 14-16, 1977
Ann Arbor, Michigan

AGENDA

WEDNESDAY, September 14: Assembly Hall, Michigan Union

9:00 a.m. General Session

Presiding: -Geraldine T. Scholl
The University of Michigan

Welcome: -Frederick W. Bertolaet
Associate Dean, School of Education
The University of Michigan

Keynote Address: "Childhood Visual Impairment: Sources and Uses of Knowledge"

-David Warren
University of California

10:30 a.m. Coffee

10:45 a.m. Reactor Panel:

-Allan Dittman, Bureau of Education for the Handicapped
-Emerson Foulke, The University of Louisville
-Philip Hatlen, San Francisco State University
-Rosemary O'Brien, Montgomery County Public Schools

11:45 a.m. Lunch

1:00 p.m. "How can doctoral research contribute to a comprehensive research effort?"

Moderator: -Edna Adelson, The University of Michigan

Presenters: -Janet Fletcher, Ontario Institute for Studies in Education
-Daniel Head, The University of Northern Colorado
-Ronald Schnur, The University of Michigan
-Ron Texley, San Francisco State University
-Amanda Hall, San Francisco State University

2:15 p.m. Coffee

2:30 p.m. Respondents:

-Joan Chase, Rutgers Medical School
-Susan Spungin, American Foundation for the Blind

General Discussion

6:00 p.m. Buffet supper at: 1400 Country Club Road, Ann Arbor

THURSDAY, September 14: Rackham Building, West Conference Room - 4th Floor

9:00 a.m. Current Research: Problems, Issues and Next Steps

Presiding: -Nancy Bryant, Michigan School for the Blind

Presenters: -Randall Harley, George Peabody College
-Beth Stephens and Carl Grube, The University of Texas at Dallas
-Iain Davidson, The Ontario Institute for Studies in Education

10:15 a.m. Coffee

10:30 a.m. Panel: Research and Practice: Can they be coordinated into an overall model?

Presiding: -William C. Morse, The University of Michigan
-Edward P. Berla, The University of Louisville
-John Hagen, The University of Michigan
-Jeannie Johns, The University of Michigan
-Carson Nolan, The American Printing House for the Blind
-Don Walker, The University of Virginia

11:45 a.m. Lunch

1:00 p.m. General Session

Preparation for work sessions: Ronald Schnur, University of Michigan

2:15 p.m. Coffee

2:30 p.m. Work Group Sessions

FRIDAY, September 16, Rackham Building, West Conference Room

9:00 a.m.	General Session
Presiding:	Lou Alonso, Michigan State University
	Reports from Work Groups
10:15 a.m.	Coffee
10:30 a.m.	General Discussion of Reports
11:45 a.m.	Lunch
1:00 p.m.	"Where do we go from here?"
	-Josephine Taylor, Bureau of Education for the Handicapped
3:00 p.m.	Adjournment

SYMPOSIUM - RESEARCH ON EARLY CHILDHOOD VISUAL IMPAIRMENT

Evaluation Form

Check one: 8 University Faculty
____ Student
____ Agency
____ Other (specify)

I. Please rate each of the following sessions relative to contributing to the goals of the symposium. Include specific comments where appropriate.

WEDNESDAY a.m. - Keynote address (Warren)

1	2	3	4	5
			XXX	XXX

Little — A Great Deal

Comments:
Very, very good!
This was a new high (in every way) for meetings in our area. Pertinent, timely, well-prepared, well-articulated, thought-generating.
Reactor Panel (Dittman, Foulke, Hatlen, O'Brien)

1	2	3	4	5
	X	XX	XX	X

Little — A Great Deal

Comments:
Good.
Refreshingly diversified.

WEDNESDAY p.m.

Doctoral Student Session: (Adelson, Spungin, Tuttle and Students)

1	2	3	4	5
X		X	XXX	X

Little — A Great Deal

Comments: Written comments from a wide range of students would add much more.
Good to hear others' problems.

SYMPOSIUM - RESEARCH ON EARLY CHILDHOOD VISUAL IMPAIRMENT

Evaluation Form

Check one:
___ University Faculty
__6__ Student
___ Agency
___ Other (specify)

I. Please rate each of the following sessions relative to contributing to the goals of the symposium. Include specific comments where appropriate.

WEDNESDAY a.m. - Keynote address (Warren)

1	2	3	4	5
			X	XXXXX

Little — A Great Deal

Comments: Definitely set the tone or goal for the symposium!
Warren did a good job!
Highly appropriate, relevant. Stimulating and productive presentation. Provided a great deal of useful information about research and future goals needed.

Reactor Panel (Dittman, Foulke, Hatlen, O'Brien)

1	2	3	4	5
		X	XX	XX

Little — A Great Deal

Comments: The reactor's comments are very valuable and interesting. I like Dr. Foulke's comments.
Particularly useful comments from Foulke and Hatlen.

WEDNESDAY p.m.

Doctoral Student Session: (Adelson, Spungin, Tuttle and Students)

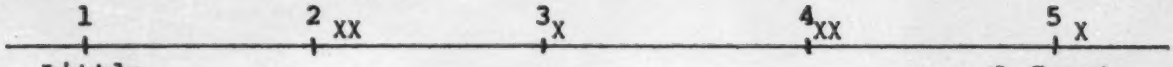
1	2	3	4	5
			XXXX	XX

Little — A Great Deal

Comments: An opportunity rarely afforded students to interact and get feedback from each other.
Discussion (?) session that followed developed into a dialogue of limited interest to remainder of group.
Students were in the main caught off guard so the comments did not tie togtehr well. By and large the feelings expressed were relevant and appeared to be well received.
Helpful to know what other students are doing-their perspectives and ideas.

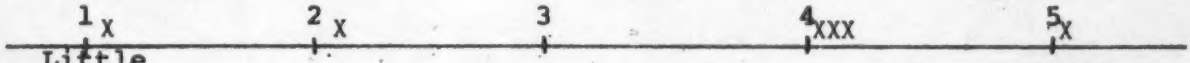
THURSDAY a.m.

Current Research (Harley, Stephens - Grube, Davidson)



Comments: Introduction was too long even if amusing. Time for each speaker should have been allotted before the first speaker, not after she had consumed a disproportionate amount of time. The speakers seemed to have different ideas about the aims of the session- perhaps this had not been clearly specified.
Not enough time in this session.

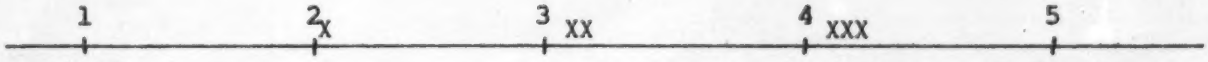
Panel (Morse, Berlá, Hagen, Johns, Nolan, Walker)



Comments: I liked Dr. Morse and Dr. Hagen's comments
Stimulating.
Particularly useful comments from U. of M. staff.

THURSDAY p.m.

Work Groups



Comments: Group seemed unwilling to come to closure on the topic.
The charge seemed rather large in view of available time. It might have been helpful to provide a specific group topic.

II. Please rate the extent to which the Symposium overall achieved the three stated goals:

1. Appraising the present state of research in childhood visual impairment:

Comments: This symposium let me understand where I stand and where other people stand and gave me a sort of overall vein about what has been done and what needs to be done.

The symposium has highlighted the field's strengths and weaknesses in the research area.

2. Determining needs, setting priorities and developing guidelines for more comprehensive coordinated research efforts in the future:

1	2	3	4	5
	X	X	XX	X
Little or none				A Great Deal

Comments: Development of guidelines for more comprehensive coordinated research efforts in the future remain extremely sketchy.

3. Arriving at a proposal or recommendations for coordinating research efforts:

1	2	3	4	5
	X		XXX	
Little or none				A Great Deal

Comments:

III. Any other comments about the Symposium, future directions, etc., would be appreciated.

Of greatest importance to me was being able to rub shoulders with other grad students interested in the visually impaired, and to meet the "established" people in the field. I found the symposium incredibly well-organized - from the first letter sent out, to the last day's coffee and donuts! You Michigan folks couldn't have been more hospitable: many thanks! On "list of participants" would like to have seen in every case the individual's title or position, e.g. "department chairman" or "graduate student". Would like to have seen somewhat less emphasis on education and more on cognition, O&M, daily living skills and on research in these areas.

I think this is a very successful symposium. I enjoyed very much talking to those people whose articles I read on the journals and got some first hand information and advice.

It might have been helpful for Josephine Taylor to present earlier in the meeting concerning funding, OE programs, etc.

The symposium seemed directed beyond early childhood research, however, there remains a need for unified research efforts in other areas of the VH.

I found the symposium to be very exciting and worthwhile, particularly as a Ph.D. student. It was helpful to learn more from other students about their research direction, university programs and advisors. David Warren provided some specific needs and ideas toward future research that hsould be developed and integrated into university programs and their direction. Many thanks for the invitation.

SYMPOSIUM - RESEARCH ON EARLY CHILDHOOD VISUAL IMPAIRMENT

Evaluation Form

Check one: ____University Faculty
____Student
__2__Agency
____Other (specify)

I. Please rate each of the following sessions relative to contributing to the goals of the symposium. Include specific comments where appropriate.

WEDNESDAY a.m. - Keynote address (Warren)

1 2 3 4 5XX

Little — A Great Deal

Comments:

Almost too extensive - some ivory tower naivete about real world, i.e., subjects. Creative, relevant, scholarly.

Reactor Panel (Dittman, Foulke, Hatlen, O'Brien)

1 2 3 4X 5X

Little — A Great Deal

Comments:

Too little time!

WEDNESDAY p.m.

Doctoral Student Session: (Adelson, Spungin, Tuttle and Students)

1 2X 3 4X 5

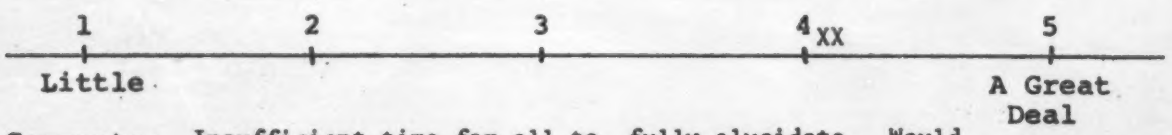
Little — A Great Deal

Comments:

Interesting, but not inspirational.

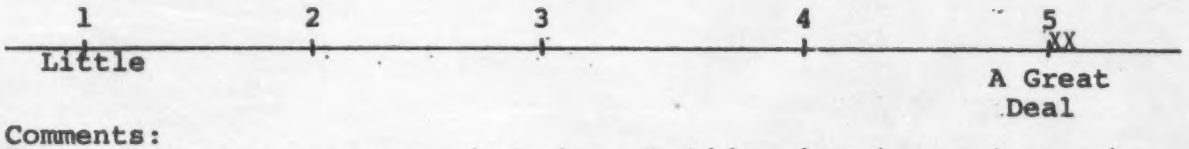
THURSDAY a.m.

Current Research (Harley, Stephens - Grube, Davidson)



Comments: Insufficient time for all to fully elucidate. Would have liked pre-meeting summary of research - so presentors could have focused on more relevant concerns.

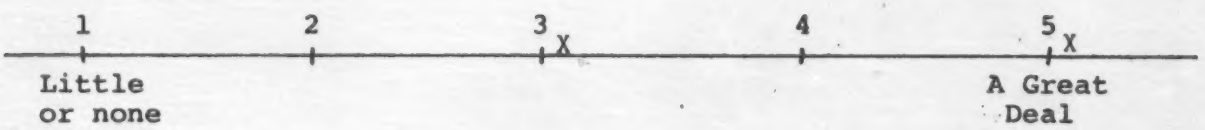
Panel (Morse, Berlá, Hagen, Johns, Nolan, Walker)



Comments:
More position type comments - stimulating. Would have been interesting to give all participants five minutes of floor time to comment, raise issues, etc.

THURSDAY p.m.

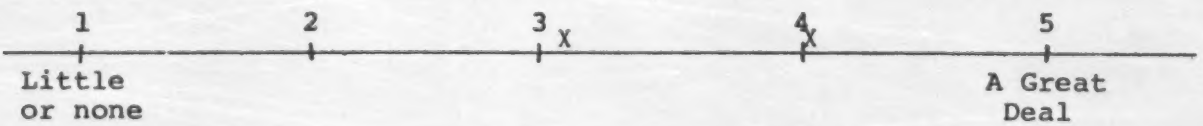
Work Groups



Comments:
Needed more of these.
Fun - remains to be seen if research and products do evolve and are useful.

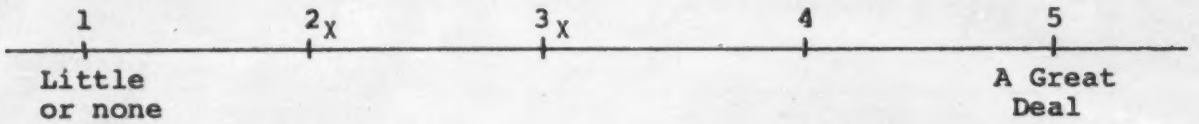
II. Please rate the extent to which the Symposium <u>overall</u> achieved the three stated goals:

1. Appraising the present state of research in childhood visual impairment:



Comments:

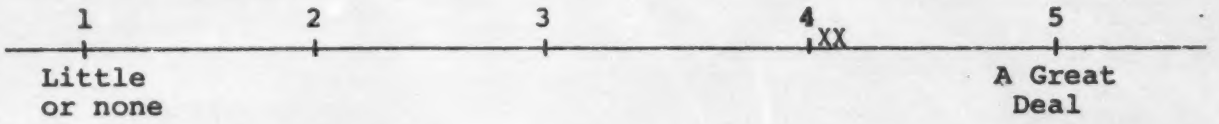
2. Determining needs, setting priorities and developing guidelines for more comprehensive coordinated research efforts in the future:



Comments:

May not be possible on scale implied.
May not be adviseable.

3. Arriving at a proposal or recommendations for coordinating research efforts:



Comments:

Ron's charge too complex.
May not be possible on scale implied.
May not be adviseable.

III. Any other comments about the Symposium, future directions, etc., would be appreciated.

It probably is unrealistic to expect to accomplish all of these goals in one conference and I hope that the recommendations for an agency, advisory committee, or council will materialize. You are to be congratulated on the selection of a diverse, knowledgeable and talented group. Arrangements, hospitality - everything outstanding!

Better preplanning of details creates better image to participants. Better time lines, for requesting participation in specific program roles. Better guidelines for expectations, etc.

SYMPOSIUM - RESEARCH ON EARLY CHILDHOOD
VISUAL IMPAIRMENT

Evaluation Form

Check one: ____University Faculty
____Student
____Agency
__1__Other (specify) Research

I. Please rate each of the following sessions relative to contributing to the goals of the symposium. Include specific comments where appropriate.

WEDNESDAY a.m. - Keynote address (Warren)

1 2 3 4 X 5

Little — A Great Deal

Comments:

Reactor Panel (Dittman, Foulke, Hatlen, O'Brien)

1 2 3X 4 5

Little — A Great Deal

Comments:

Not enough range of opinion

WEDNESDAY p.m.

Doctoral Student Session: (Adelson, Spungin, Tuttle and Students)

1 2X 3 4 5

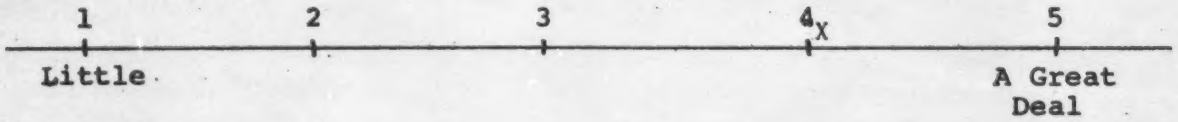
Little — A Great Deal

Comments:

People not prepared to present their views.

THURSDAY a.m.

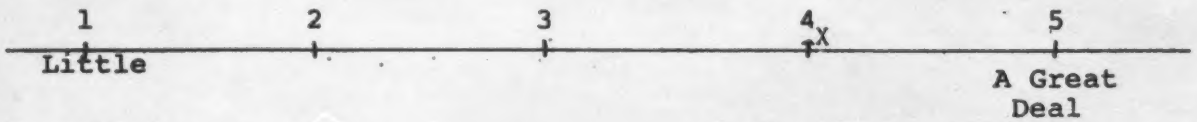
Current Research (Harley, Stephens - Grube, Davidson)



Comments:

Too rushed - not enough time for interaction.

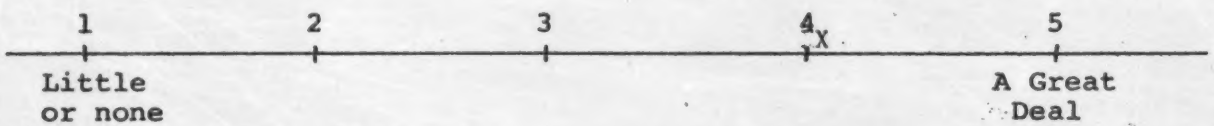
Panel (Morse, Berlá, Hagen, Johns, Nolan, Walker)



Comments:

THURSDAY p.m.

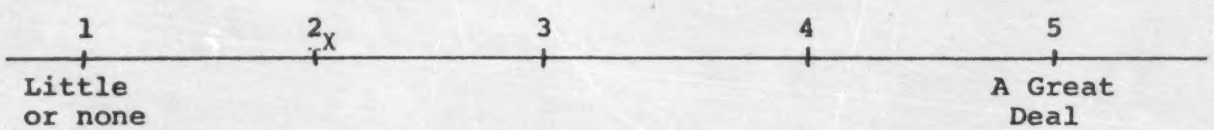
Work Groups



Comments:

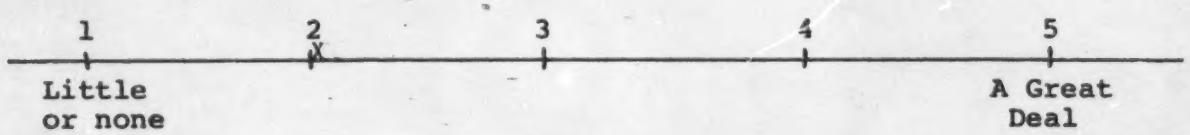
II. Please rate the extent to which the Symposium overall achieved the three stated goals:

1. Appraising the present state of research in childhood visual impairment:



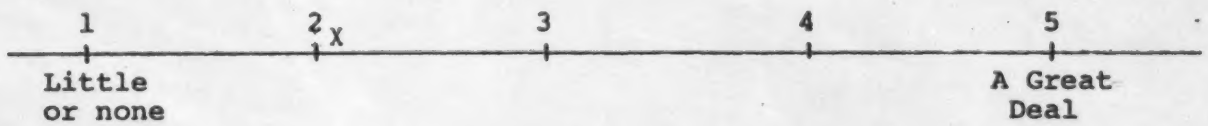
Comments:

2. Determining needs, setting priorities and developing guidelines for more comprehensive coordinated research efforts in the future:



Comments:

3. Arriving at a proposal or recommendations for coordinating research efforts:



Comments:

III. Any other comments about the Symposium, future directions, etc., would be appreciated.

There was a need for more specific direction. The goals were not clearly defined, nor were the avenues for accomplishing the formation of such goals specified.